new species. The species which he considers my *Fusus curtus* is very different from the *F. Sabini* of Gray, or the *F. togatus* and *F. Pfaffi* of Mörch (all enumerated by Friele as synonyms); and I regard the last-named three species as the *F. ebur* of Mörch and not as my *F. Sarsi*. However, notwithstanding any trifling errors, if they be errors, the work of Herr Friele is not only admirable and valuable, but is imbued with that scientific merit and modesty which are peculiar to our fellow-workers in Scandinavia; and we shall look forward with great interest to the continuation of his papers on the Mollusca of the Norwegian North-Sea Expedition.

J. GWYN JEFFREYS

Tables for the Use of Students and Beginners in Vegetable Histology. By D. P. Penhallow, B.S., late Professor of Chemistry and Botany in the Imperial College of Agriculture, Japan. (Boston, 1882.)

THIS little work by no means meets the expectations which its title arouses. The author states, indeed, in his preface that the scope of the work is purposely limited, but the limits are so narrow that the work will not be of much use to the student who has a competent teacher, and it will not be of any use to the beginner who is attempting the study of vegetable histology by himself. The book deals simply with the micro-chemistry of plants; the reagents are enumerated, as are also the various substances to be met with in the cells, but no attempt is made to give an account of the mode of application of the reagents for the detection of the substances, and in certain important cases (the chloriodide of zinc, for example) the mode of preparation of the reagent is not given. Not a word is said about imbedding, nor is any mention made of staining. The general mode of treatment of the subject is thoroughly unpractical. For example, silica is said to appear in plants "as a transparent deposit"; but every histologist knows that the silica in a cell-wall can only be made evident by incinerating with nitric acid.

The priority which the author claims can hardly be granted in view of the fact that Poulsen's valuable "Microchemie" has been in the hands of European histologists for several years. The selection of literature given at the end also betrays the author's want of acquaintance with his subject, inasmuch as no mention is made of such important works as Dippel's "Mikroskop" and De Bary's "Vergleichende Anatomie."

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

The Matter of Space

IN his paper on "The Matter of Space," in NATURE, vol. xxvii. p. 349, Mr. Charles Morris has given us an excellent exposition, and, as I believe, in general a perfectly correct one, of the fundamental laws and properties of matter and motion. But as I have for some time been investigating the views which he describes with exactly the results and consequences at which he has arrived (excepting only in one material difference to which I will presently return), a little outline of the mathematical form which I found that the discussion of the subject could receive, and to which it was accordingly submitted in my examinations of its scope and contours, will aid readers of Mr. Morris's paper, perhaps, in attaching clear ideas to some of the expressions which he uses, and in thereby discussing and estimating very easily and fairly the positive truth, in general, or in a few points, of the paper's considerations, the just degree of reliability

at all events, which the marvellous maze of internetted motions possesses, which he has most tersely and graphically, and at least in the main, as it appears to me, correctly and truthfully described.

Angular momentum, or (for a particle of unit mass) the rate of description of sectorial areas, is, like actual energy, a quantity of two dimensions in space; it is in fact the vector-product of (or the quadrilateral area between) the two radii of the particle's orbit and hodograph. Tractive momentum, or the product of the unit-particle's radius-vector and the resolved part of the particle's velocity *along* (instead of across) the radius-vector, is equally a quadratic product (but differently estimated) of the two foregoing orbit and hodograph radii. It is not the rate of description of *an area*, like angular momentum, but the time-rate of the square on the orbit-radius. The time-rates of each of these momenta are similar to them in space-relation, and are respectively angular moment or twirl (of a force-couple) and tractive moment or wrest (of a motor-couple). But if a small step of angle is the ratio of a circular-arc step (or of a small step along its tangent) to the circle's radius, this being numerical, a twirl's work through this small step of angle is similar in space-relation to the twirl itself and to its time-effect, or angular momentum.

The same similitude in space-relation will exist between a wrest, or motor-couple, and its time-effect (or tractive momentum), and its small step of work, if, in imitation of the practice for a twirl's or force-couple's action, a wrest's space-step is defined to be the ratio of the particle's *step along* the radius to the orbit-radius. This counterpart of angle-step may be called a traction-step; and it is the small percentage of elongation which the radius undergoes. If this construction is assumed, there ensues from it a close, and evidently significant, analogy between the time-rate of *orbit-radius* square (which denotes at once, in space-relation, a *motor*-couple and its time- and space-effects) and the *hodograph-radius* square (which expresses simultaneously in space-relation a *force*-couple and its time- and space-effects). Although the square of the hodograph-radius signifies the square of the material point's velocity, or its *directed* actual energy, I conceive that the square of the orbit-radius represents a square of un-directed velocity, or an undirected energy of "higgledy piggledy" motion of the material point; and its time-rate is *a horse-power of* the point's quaquaversal, or undirected actual energy. Viewed in this light, twirls or force-couples and their time- and space-effects are all graphically synonymous with actual directed energy; but wrests or motor-couples and *their* time- and space-effects are all graphically synonymous with *horse-power* of *un-directed* actual energy. For these latter quantities Mr. Morris uses indifferently the various words, "momentum," "heat momentum," "heat velocity," "heat," "motor energy," "heat energy," "heat vibration," "centrifugal energy," and "centrifugal or motor vigour," of a moving point; but while they are all, as he rightly opines, convertible quantities in their relation to graphic space, yet the theory of force-couples with which (*mutato nomine*) they are equally convertible in the same space, teaches us that a twirl-group falls mechanically, according to its association with time and angle, into three distinct divisions, of an action (the couple) and its time- and space-effects (angular momentum and accumulated work). It is so also with the motor-couple's graphic-space measure, "vigour." In proper combinations with time and traction-ratio[1] it becomes either an action or a kind of momentum or a form of work. But in discussing these new quantities' properties two maxims of construction and interpretation must be kept constantly in view.

In the first place, we must not expect a motor-couple (although it tends to alter ϕ) which endows a point with undirected horse-power, to tend to lengthen or shorten the point's radius-vector *in the same way* that a force would do. If by their actions motor-couples can *in any way* oppose the action of a force or force-couple, it must be, not by exerting force themselves, but by giving rise to force where they act. Now motor-couples can no more act intelligibly upon a single point (to range a radius's extremities towards or from each other) than a force-couple can (to turn a radius's two ends round each other). Hence motor-couples must produce force in a material point in virtue of the point's being an aggregation of material points, or in other words the appearance of force is a sign of the compositeness of the material point upon which it acts. *Per contra*, forces can produce force-couples, or

[1] The integral of traction-ratio, $\int d\phi = \int \dfrac{dr}{r} = \log \dfrac{r}{c} = \phi$; I identify with Rankine's "thermodynamic function" (for which he uses the same symbol, ϕ) usually termed "entropy" in works on thermodynamics.

if properly combined can balance them, on a collection of material points, if certain internal conditions (always including conservation of force-effects and conservation of twirl-effects) of the component points' mutual force and couple-actions on each other, which we call certain static relations of the system, are fulfilled ; and then we have forces on such an aggregation either giving rise to or holding in check force-couples acting on it. But no combination of force-couples, on the other hand, can either produce in the system, or resist in it, the action of a single force.

Now as a motor-couple and its parts exert time-flow of one form of energy, they differ from a force-couple and its parts in the same way that these differ from uniform rotation and translation ; and as it happens that while rotations can combine on a system to produce translation, and not the opposite arrangement, and just the reverse of this relation prevails in force-couples and their forces, so we may infer that in a system of connected points motor-couples would have the opposite property to force-couples, and in combination together on the system, instead of being produced by, they would either wholly or partially produce, a form of resultant of the nature of a motor-couple part. This kind of resultant, too, will exert a tendency on the system as a whole, with equal and similar intensity at all its points. Such a combination of motor-couples on a body, therefore, will in general communicate to it by their conjunction, not horse-power of undirected actual energy in the same manner as a single couple would, but some or no resultant couple-part, and some or no resultant couple, just as a set of forces, (or rotations) applied to a body may yield a mixed resultant of a force and couple (or of a rotation and translation), the couple in one case and the translation in the other both taking effect upon the body as a whole, since each is quite devoid of any particular point of application in the body. This property, which we may reasonably assign to motor-couples, of furnishing in combinations on a group of material points a dual resultant in general, and the condition that they exert singly a time-flow of undirected energy, are together the first maxim to be kept in view in discussing their effects ; for the double-resultant's nature, of a congregation of motor-couples, in general resembles that of a screw's motion, which is partly translative along and partly rotative round a polar axis. Along a given line through the system therefore this resultant acts jointly, partly as a wrest, or residual motor-couple, and partly as a couple-half, of whose nature and effects no attempt, in what precedes, has yet been made to give an explanation.

Although such views as these of matter and motion largely invite investigation, it is rather their conformity to observation and to such slender mathematical evidence as is derivable from the laws of graphic space than any rigid demonstration of their validity which has led me to put faith in them. Where time and entropy (which linear dilatation is above surmised to be) clasp and bind undirected energy in new ties of space, so singularly like but yet distinct from the well-known ones which regulate the transformations of directed energy, intrusion into the mathematical avenues of the problem is almost warded off by the obduracy of the new inquiry, and only scattered fields of cultivation, occurring ever and again along his road, assure the venturesome wanderer in the new tract that the course before him still always lies in habitable regions.

It would be presumptuous therefore to insist, until the mathematical field has been thoroughly explored, upon a preference of one view or hypothesis of motor-actions, as decidedly superior to another; but adopting, as Mr. Morris does, the opinion that the effects of motor-actions are conserved, and adding to this an assumption that in groups of points subjected to them the mutual conservation may not be (as it always is among the mechanical connecting forces of every piece of ponderable matter) perfect and complete in the system by itself, *without* reckoning on to it one other external point, then a material simplification of the views unfolded in his paper would, I believe, be introduced, by adopting a different hypothesis from that which Mr. Morris advances of the nature of the ether as an exceedingly attenuated form or "fourth state " of gross "gravitating" (or ponderable) matter.

If Nature's course could be retraced to the beginning of time, we may suppose in that *gouffre* of antiquity ether to have been differentiated from gross matter in this way, that whereas internal conservation of motor-effects suffices to weld a group of material points into a resultant yielding system, then, no limits of smallness being imposed upon the group, it is allowable to define a point
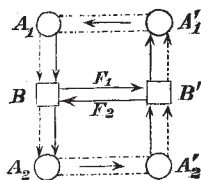
(in the language of graphic space) of gross or ponderable "gravitating" matter as an originally differentiated mass of aërilian points, upon which the dual resultant of the conjoined motor-couples on the aërilian points will take effect. A part of this resultant is a couple-half, about which we know nothing ; and we may reasonably suppose it to be attractive and repulsive force acting on the baric point. The other part of the resultant is an unbalanced motor-couple, only susceptible of conservation as to its effects by an equal and opposite one in some other similar mass or aërilian assemblage. A certain integrity can, I conceive, be imparted to this first hoard of scaffolding of the new theory's construction, by locating the conserving couples, of which the motor-couples supposed to be aboriginally welded together are the counter-equivalents, not in a single, but partly in one, and partly in another, set of free-moving aërilian points in such a way that, while the resultant motor-*couple* is balanced by the first set, the *force*-resultant of the massed couples' combination, will be balanced *through* a counter-equivalent force-resultant in another mass-point *by* the free motor-couples of the other aërilian set. The residue of this set's couples will be occupied in opposing the unbalanced couple-residue of the couples massed together in the second baric point, while these couples' transmittent force-resultant will be opposed *by* the still uncompensated portion of the motor-couples acting on the first free-moving set. Perfect compensation of the two dual resultants cannot then take place under these conditions, without exact counter-equivalence of the half-couple (or force-) resultants, and therefore also exact counter-equivalence in their native state between the two groups of motor-couples acting on the two free aërilian sets ; at least, if we assume massed and moored aërilian points to have been all originally endowed in pairs with equal counter-couples, and if their modes of collection into mass-points and of producing force-resultants were aboriginally all alike.

In our present undeveloped knowledge of the mathematical properties of tractive or motor-couples, and of their random-energy horse-powers' geometrical relations to the common mechanical modes of exertion of directed energy in forces and couples, it would be premature and vain to speculate as Mr. Morris does, I believe too boldly and fearlessly, in his paper, upon Nature's established order of progressive collection of baric points into " spheres," or into the atoms and molecules which further build up atmospheres, suns, planets, and all ponderable bodies. My views diverge here from his in, at least, one salient point, that the ether (as we must still in sober science term his " interspheral matter ") is held, in his opinion, to be ponderable or " gravitating," and to be endowed with a vigour of motion which exempts it from yielding to its vigour of gravitation. By thus identifying " interspheral matter's " or the ether's particles with those of matter " employing its motion secondarily about new centres of gravity " (of *really* gravitating or ponderable " spheres "), the way is barred at once of explaining the ultimate sources of attractive and repulsive force by exercises of motor " vigour." But further than this we must evidently abandon definitely all reasonable hope of constructing out of particles' " incessant leaps in nodes of an interminable network of motions, affecting in long motor lines myriads of interspheral particles," any intelligible framework of the important laws, of radiation, magnetism, and electricity which we know that a clear comprehension of the " interspheral " ether's real constitution would immediately unfold to us, if its real nature and that of its relations to ponderable matter were rightly understood. In the form therefore in which Mr. Morris's theory presents itself to us, it fails completely (by only the slightest possible illusion, as I venture to submit, in the choice of its principal hypothesis) in attaining the admirably well pursued and well nigh compassed object of its otherwise exhaustively clear and excellently propounded arguments and demonstrations.

In the view which I have here advanced, massed assemblages of aërilian points form irrevocably the points of gross or ponderable matter, while an equal number of moored points, inseparably connected two and two with the former ones, form bound aërilian assemblages equally untransformable and forming active individual parts of the unchanging ether. That the latter points, unlike those of the massed group, may rove at large in graphic space, does not preclude them from all occupying a common point in another space domain, just as a number of balloons may be all at one height, whatever the courses of their tracks upon a map may be. Nor, again, does an encounter of two balloons' courses on a map necessarily entail collision between the two balloons, since at the time they may be at different

heights. It is thus quite conceivable that, in a scale of space foreign to our graphic measures, the free roving aërilian set may all occupy a common place in this foreign scale of space, and that a massed and a moored aërilian point may have the same position in graphic space without impinging on each other, as such points are not at the time at one and the same place in the foreign space. The moored or bound ether may thus traverse the space occupied by the massed ether of gross matter without mutual interference ; but, whether superposed or not in ordinary space, the pair of ether sets which compensate resultant actions of two gross-clustered sets of a pair of points of baric matter, will form, however they may mingle graphically, two orbs of ether exerting each (corporately) exactly counter-equal free-orb couples.

If for example the baric points B B′ are urged towards each other with a ponderomotive force or flow of ordinary baric momentum F_1, by the motor actions on them of two ether-orbs $A_1 A_1′$ in counter equal intensities, force-momentum only will be

transferred from A_1 to $A_1′$; while the tractive momentum (or as we may presume, the heat-energy) accompanying it will only be transferred from A_1 to B, and a similar transfer of thermal energy or tractive-momentum will at the same time take place from B′ to $A_1′$.

Should it, in the next place, be required to oppose the action F_1 by an equal counter-force F_2, a pair of ether-orbs $A_2 A_2′$ must be superadded to those already urging B and $B_1′$, so as to urge them in the opposite direction. It will be seen from the figure that the total effect of this and of the previous orb-pair's actions will simply be that the pair of ether-orbs connected with B will transmit motor energy from one to the other (from A_1 to A_2), and the other ether-pair will also transfer an equal amount of energy contrariwise from one orb to the other, without any leakage of ordinary momentum occurring at B and $B_1′$, by the neutralised action, into the channel B B′.

Newcastle-on-Tyne, February 10   A. S. HERSCHEL

(To be continued.)

Terrestrial Radiation and Prof. Tyndall's Observations

IN NATURE, vol. xxvii. p. 377, I see a notice on Prof. Tyndall's observations on terrestrial radiation, with the author's concluding remarks, that meteorologists should not be offended by his saying that from outsiders equipped with the necessary physical knowledge they may expect valuable aid towards introducing order and causality among their observations. May I be permitted to state that Prof. Tyndall will give no offence, at least to the meteorologists whose works are advancing this science at the present time.

Prof. Tyndall tries to prove by his observations the extreme importance of vapour of water as a check to terrestrial radiation, and he mentions the much greater difference between a thermometer in the air four feet from the ground and another on cotton wool on a morning when snow was lying on the ground than on other nights, equally clear, but with higher temperatures of the air and no snow. Now it is well known that, *pari passu*, a surface of snow will be colder than a surface without, because (1) snow is an excellent radiator ; (2) because, as a very bad conductor, it shelters the surface from the influence of the higher temperature of the soil. In the observation on December 10, the thermometer on cotton wool was so cold because it was under the influence of the cold radiated by the snow, and besides immersed, so to say, in the coldest stratum of air near the ground. To my mind, the manner in which the observation was conducted does not prove what Prof. Tyndall advances. To isolate, so to say, the influence on radiation of the atmosphere itself, he should have placed, between two poles, at some feet above the ground, a plank, and on it his cotton wool and thermometer. No doubt that this thermometer, isolated from the snow, should have shown a higher temperature than his thermometer placed on the surface on cotton wool.

Prof. Tyndall lays great stress on the fact that the difference between the temperature in the air and on the ground was less in clear nights with a higher temperature and greater quantity of vapour of water in the air, and sees in this a confirmation of his opinion on the great influence of vapour of water in checking radiation. I do also see in this the influence of vapour of water, *but not of its absolute quantity*, but of *relative humidity*. Once the dew-point is attained, the cooling of the thermometer on the ground is arrested. The whole question between Prof. Tyndall and many physicists and meteorologists is this : nobody negates the influence of vapour of water on terrestrial radiation, but Prof. Tyndall ascribes this influence to vapour in the gaseous state, while his opponents hold the opinion that in this state vapour of water has a diathermacy scarcely different from dry air, while, condensed in small ice crystals or water droplets, it really interposes a very efficient screen to terrestrial radiation, even if, which sometimes is the case, it is perfectly transparent to light, *i.e.* invisible to our eye. Another influence of water on terrestrial radiation is admitted by all : that is, that of the latent heat in the deposition of dew and hoar frost.

If we wish to make meteorological observations bearing on the question, the following *modus operandi* should be adopted : (1) observations should be made in climates where, with a tension of vapour greater than that which obtains in England in winter, the relative humidity is yet so small that there is no dew on clear nights, or at least it appears rather late ; (2) three thermometers placed on cotton wool, but at different heights above the ground should be observed, say one on the ground, and the others at heights, say from 10–100 feet above.

If Prof. Tyndall's views are right, the highest of the thermometers should show by far the lowest temperature, as it is not screened from radiation by the vapour of water diffused in the lowest stratum of air. I think every meteorologist will express the opinion that there will be scarcely a difference in this case. As to the observations in different climates, those made where the relative humidity is low should give no greater difference between the thermometer in the air and on the wool than the observations in England on clear nights, with the same vapour tension, if Prof. Tyndall's hypothesis be admitted. I think we have already many observations which prove that, with vapour-tensions much above 0″·181 (or 4·6 mm.), *i.e.* above that of saturation at 32° F., terrestrial radiation is very great, if only the sky is clear and the relative humidity small. No doubt the decrease of the temperature of the air from the midday maximum to the night minimum is caused by terrestrial radiation. I give some figures from the observations at Biskra, in the Algerian Sahara.[1]

	Difference of daily max. and min.	Mean temperature.	Tension of vapour.	Relative humidity.	Amount of cloud.
January ...	25·4	56·8	0·264	61	1·6
August ...	39·2	89·6	0·557	40	0·8
October ...	35·6	68·4	0·432	58	0·9

In an arid climate in low latitude the non-periodic variations are but small, and the difference between the maxima and minima is very near to the daily range of temperature. As the amount of cloud is very small in all three months taken here, the conditions for terrestrial radiation are very favourable. If vapour of water in the gaseous state impeded terrestrial radiation so much as stated by Prof. Tyndall, we should expect to find the daily range smaller in August than in January, on account of the double amount of vapour in the air. The reverse is the case, the daily range being by 14·8 greater in August than in January. Has anybody observed a daily range of 39·2 in England, be the amount of cloud and the vapour-tension ever so small ?

I must add that in all observations bearing on terrestrial radiation we must not forget that other substances besides water in its three states may interpose a screen to radiation. I mean especially dust and smoke of all kinds. Now far from large cities, there are many reasons why in winter, especially when the ground is covered with snow, the air will hold less of these impurities than in summer, as in winter there are no fires of forests and peat-bogs, there is little inorganic dust, because the humidity of the soil, and still more so the snow, prevent it ; organic dust, germines, &c., are also absent, or present in very small quantities, on account of the small amount of plant and lower animal life. The absence of dust and smoke explains the great purity of the air in winter, so favourable to solar and terrestrial radia-

[1] "Annales du Bureau Central Météorologique de France," 1879, vol. ii.